SCIENCE

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THE NEED OF PSYCHOLOGICAL TRAINING.

A FEW — only a few — years ago we learned psychology from antiquated text-books that by tales of extraordinary occurrences, quotations of poetry, emphatic assertations, occasional proofs by the phrase "it is evident," and a few improperly observed facts, gave a complete exposition of the human mind in 500 or 600 pages — except in some cases where the author was kind enough to be satisfied with half that amount. To day a psychologist of that kind lectures to bare benches in the universities of Germany, and the new psychology has got such a hold in America that it is rapidly becoming a fashion, if not a fad.

Still, in this very fact there lies a great danger to the proper development of the science. There is a tendency to careless work, to rapid shuffling off of quasi-experimental researches, to a neglect of the drudgery of a scientific investigation of the fundamental problems, and to a pursuit of ghost stories, telepathy, and sensational hypnotic tales. Even where the psychologist is really a scientific man there is a tendency to rest contented with merely qualitative results where quantitative measurements could be made with the exercise of brains and patience.

In regard to the sensationalism and quackery that have assumed the garb of psychology we can do no more than every other science does in that respect, simply put the public on its guard. If, as is usually the case, the public prefers swindle to science; the matter is beyond our control. There is also little to be said against the so-called "theoretical" or "metaphysical" psychology that has blocked scientific development in the past and opposes it in the present. The "metaphysical" psychology is neither metaphysical nor psychological; the term is used merely to cover up the inability or the dislike for careful observation and experiment, it being much easier to sit at home in the study chair and spin out a work on psychology than to put on the apron, clean batteries and smoke chronograph drums in the laboratory.

What is to be called to attention here is the fact that we psychologists are not making the proper efforts toward exactitude in our experiments. In the first place it is becoming too common to consider that going through any careless series of manipulations is making an experiment. An experiment is the systematic variation of the conditions governing a phenomenon in order to observe the results of such a variation, The amount of systematic preparation required and of careful observation to be exercised depends on the stage of development in which the science finds itself. Any lack of preparation that could have been expected, or any deficiency in the necessary care, removes the pretended experiment from the realms of science to that of dilletantism. Dilletantism may be all very good as a source of amusement, but it must never be considered as science. As Wundt has remarked, "the most dangerous enemy of psychology to-day is not the metaphysical psychology of former days, but the self-sufficient amateurism that considers every aimless toying as a scientific experiment."

Aside from this amateurism there is another deficiency, perhaps of a still more important nature. In the various periodicals we meet accounts of qualitative experiments that might just as well have been made quantitative. Of course qualitative experiments are necessary as preliminary investigations, but they are inexcusable where quantitative ones can be made. That is to say, although they are necessary as forerunners of measurements, and although at certain stages of investigation, they are of incalculable value, yet the scientist must never rest satisfied with them, but should regard them only as stepping-stones for further progress. I can find no better way of stating this than by repeating the words of Sir William Thomson: "I often say when you can measure what you are speaking about, and express it in numbers, you know something about it; but when you cannot measure it, when you cannot express it in numbers, your knowledge is of a meagre and unsatisfactory kind; it may be the beginning of knowledge, but you have scarcely, in your thoughts, advanced to the stage of science, whatever the matter may be" ("Popular Lectures and Addresses," I., 73, London, Macmillan, 1889).

The first step in an investigation is a determination of the relations of dependence between various phenomena; this, however, must give place as soon as possible to a measurement of the changes in the mutually related phenomena. This we can already do to a great extent in regard to mental processes. The degree of accuracy obtainable is in some cases scarce second to that of physical determinations, but in others it falls far below.

The future of psychology, however, lies in the possibility of increasing the accuracy of the measurements of mental phenomena. An advance in accuracy is a difficult thing; but it is of such importance that any sacrifice of time and trouble is justifiable for that purpose. To reduce the error of observations in a given problem by a tenth is a great task, and it becomes greater with each increase in accuracy. Psychology, however, is in the fortunate position of being in possession of methods more accurate than the majority of psychologists are able to apply. Wundt and his followers have gone ahead so rapidly that on the one hand their results can claim an accuracy only one degree less than that of physics, but on the other the psychologists who have never had a training in his laboratory are not quite able to keep up the pace. This, of course, does not apply to those domains of mind not yet subject to measurement. It is very true that there are still large groups of mental phenomena not yet investigated by experiment; we have not yet found a measure for hate, for enthusiasm, or for vertigo. There are still others in regard to which we stand at present just on the point of introducing experimental methods without having achieved anything of great importance; such are the subjects of pleasure, hallucination, the lower senses, etc. Yet again we find those that are fast yielding themselves up to qualitative and even quantitative analyses, e.g., volition in some of its results, the sense of equilibrium, pain (dermal pain quantitatively measured), smell (quantitative measurements by Zwaardemaker and Henry), etc. On the other hand the magnificent achievements in the domain of sight,

the good ones in hearing, those in the senses of pressure and temperature, the accurate measurements of visual space, the measurements of the reaction-time, etc., have all tended to place experimental psychology on a high level and to furnish a foundation for a science of psychical measurements, or psychometry.

What is the reason, then, that we are doing second-rate work when we might do first? The trouble lies, it seems to me, in the lack of a proper training. We attempt to make experiments; but how many of us have received a practical training in the use of our apparatus? We make observations; but how many are familiar with the methods of observation and the computation of errors? We obtain tables of results; but how many know how to formulate the equation expressing those results? I know that, until I was brought face to face with the question of what to do with my figures when I had got them, it had not occurred to me to remedy my deficient training by a study of the methods of expressing results. We all of us daily use light, sound, heat, electricity, etc., in our experiments; but how many are familiar with the units and the methods of measuring these forms of energy? What a psychologist must have is a thorough course of training in psychometry, or the methods of psychical measurement.

Summing up, I would say that what we need in experimental psychology is: no quackery, little amateurism, a proper estimation of qualitative work as subordinate, a transformation of the qualitative into quantitative investigations, and, as the means of obtaining all this, a thorough laboratory training. E. W. SCRIPTURE.

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THE VESICLES OF SAVI.

IN the *Archives Italiennes de Biologie*, XVI., 1891, page 216, there is a reprint from the *Atti della R. Accad. dei Lincei*, VII., 1891, fasc. 6, of Dr. Alessandro Coggi's important notice of the development of Savi's "*appareil folliculaire nerveux*" in the torpedoes. Since Savi's announcement of his discovery of these peculiar follicles on the lower surface of the torpedo, 1841–44, an extensive series of publications has been made on the subject. The anatomy has received attention at the hands of Boll, Leydig, Kölliker, Max Schultze, Müller, and others; and the nature and functions have been variously determined. Leydig made the vesicles to be one of his three classes of organs for a sixth sense; Wagner supposed them to be electrical excitants; but the majority agreed in regarding them as tactile organs. In 1888, in my work on the "Lateral Canal System of the Selachia and Holocephala," published by the Museum of Comparative Zoology, it was proved that the vesicles belonged to the lateral system, as seen on the skates and sharks, and it was shown that they were not confined to the torpedo, but were found on such genera as Urolophus, Potamotrygon, and Disceus of the rays, where they were simple rudimentary remnants of the lateral canals. My conclusions are amply confirmed by Dr. Coggi from the embryology of torpedo, in the early stages of which he traces the ventral canals, as in embryos and adults of other Selachia. He finds various stages of canal disruption corresponding with those I had figured from the Batoids above mentioned.

Dr. Coggi's assertion that the hypothesis making the vesicles of Savi a special modification of the lateral line system was first brought forward by M'Donnell, 1864, is one to which I should take exception. It must be due to misunderstanding of M'Donnell's statements. That author enumerates five structures that "may be, or have been, confounded with different parts of the lateral line system," and he describes the last one of the five as "The bodies discovered by Savi in the torpedo (*appareil folliculaire nerveux*) — which last, however, may be related to the lateral line, as I shall afterwards attempt to show." This is sufficiently involved to make his meaning very doubtful. But to prove that M'Donnell did not advance the idea of identity of follicles and lateral lines we have only to turn to the penultimate paragraph of his article, where he classes the follicles with other tactile organs, and says that they, one and all, appear to be distinct from the system of the lateral line, which, he says, has more the appearance of a cutaneous excretive organ than of one of sensation (Trans. R. Irish Acad., XXIV., 1864, read 1862, page 161). Up to the present I have learned of no proof or assertion of identity of Savi's follicles and the lateral canal system previous to that in my work of 1888.

Respecting the utility of the follicles it may be added here that my conclusions are at variance with those of all who have heretofore discussed the matter, insomuch that I consider these organs to be practically without special function, and to represent only a transitory condition of the lateral system, intermediate between functional perfection, in the embryo, and ultimate more or less complete disappearance, during the life of the individual. As the organs are absent from particular species or from older individuals, and are rudimentary and irregular when present, this seems to me the only tenable conclusion. S. GARMAN.

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BACTERIA IN DRINKING WATER.

DR. W. MIGULA (*Centralbl. f. Bakt. und Parasitenk.*, Bd. VIII., No. 12, p. 353) makes a contribution to our knowledge of this subject which is really a new departure as regards the examination of drinking water. He points out that, although considerable stress has been laid on the examination of water for pathogenic organisms, there is no reliable rule to guide the hygienist in his examinations for the ordinary saprophytic organisms and their relation to the purity of water to be used for drinking purposes. Dr. Migula washes out small flasks with bichloride of mercury; then, after rinsing them with the water to be examined, he leaves a specimen in the flask, which is plugged with sterilized cotton wadding and covered with an india-rubber cap. It is not necessary to pack the flasks in ice, as it is assumed that if any of the organisms multiply they will all do so, while if the putrefactive organisms (those that liquefy gelatine) grow more rapidly than the others, independent evidence is obtained of the impurity of the water. Cultivations are made in flat glass dishes in order to save the time required in manipulating plates and tubes during the cooling process. After examining 400 springs, wells, and streams, the author has come to the conclusion that where there are more than ten species in any sample of water, especially when these are not species ordinarily met with, the water should not be used for drinking purposes. He found that in only fifty-nine waters was this the case, but that 169 waters contained more than 1,000 organisms per cubic centimetre, sixty-six of these having over 10,000 (forty over 50,000). From these figures it will be seen that some of the sources of supply would be condemned by the old method but would be passed by the new, and some condemned by the new would be passed by the old. Migula found in all twenty-eight species,